

REVIEW OF THE WAR BETWEEN MENTALISM AND
BEHAVIORISM: ON THE ACCESSIBILITY OF
MENTAL PROCESSES BY WILLIAM R. UTTAL

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Uttal's goals in writing this book are (a) to demonstrate that the war between mentalism and behaviorism is unwinnable because both sides are fundamentally flawed and (b) to describe a new version of behaviorism that resolves the conflict. The book is generally well written and contains many interesting and important points, but the goals are not attained because of weaknesses in some of the crucial analyses. For example, key terms such as *mind* and *behavior* are not defined; the scientific admissibility of inference is denied for philosophical reasons that are not relevant to inference as actually used; the accessibility of mind is both explicitly denied and implicitly assumed; two kinds of reductionism—between and within domains—are acknowledged but the distinction is not consistently maintained; and the proposed new behaviorism ignores rather than solves the old problems.

Key words: mentalism, mind, behaviorism, reductionism

I was especially attracted by the title of Uttal's (1999) book because, contrary to my graduate training, I have believed since the early 1960s that mind or consciousness constitutes the crucial problem in psychology. My graduate training was in Hull–Spence learning theory; for example, among many other relevant courses I had five taught by Kenneth Spence and one, on logical positivism, based on Gustav Bergmann and taught by Charles Spiker. I consider Uttal's goals for this book laudable, and it contains many interesting and important analyses of mental phenomena and the various psychological approaches to them. However, in my view the goals are not attained because of weaknesses in some of the crucial analyses.

Let me get a minor point out of the way before proceeding to the crucial analyses. Although Uttal's writing style is very good, the book is poorly edited. It contains many instances of disagreement between the number of the subject and the verb in a sentence, omitted words, inappropriate words apparently left over from a previous draft, and even a contrast between “mentalism and cognitivism” (p. 180) that was evidently meant to be between mentalism and behaviorism. Also,

good editing would have taken out inconsistencies such as an assertion on page 2 that “some of the most popular theories of psychology's history . . . have been discarded” and endorsement on page 157 of assertions that old theories never die.

FUNDAMENTAL QUESTIONS
ABOUT MIND

According to Uttal, the fundamental questions about mind are whether it is scientifically “accessible” and “analyzable” (e.g., p. 2). However, I would say that the fundamental question is “What is mind?” Uttal's questions cannot be addressed until this question has been answered; we cannot know whether we have accessed and analyzed mind if we do not know what we are trying to access and analyze. Unfortunately, the nearest Uttal comes to addressing this question is to assert that the “reality” of mind is given by personal experience (p. 2; his quotation marks). The nature of this reality is not clear, because Uttal does not specify the nature of the personal experience and because instead of providing a clear definition of mind, he offers synonyms such as “mind,” “consciousness,” “cognition,” the “stuff of self-awareness” (p. 2; his quotation marks), and “personally experienced awareness” (p. 2). Thus, the best he does is to say that whatever it is, it is real (pp. 2, 183).

Because the key concept is not defined, Ut-

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tal's analyses contribute few if any advances to the enormous psychological literature on mind. The reason little progress has been made during the 120 years or so of scientific psychology seems to me to be that mind or consciousness was reified in the questions that were asked and that Uttal still asks. I will return to this point in a later section, in which I discuss an alternative approach.

THE DEFINITION OF MIND

The Approach in Classical Psychology

Watson (1924–1925, p. 3) pointed out that the structural (classical) psychologists never really defined mind, or consciousness. They generally acknowledged—and some lamented—the lack of precise definitions, and many of them said the concept is not definable. For example, (a) William James (1890, p. 225) said, “Its meaning we know so long as no one asks us to define it.” (b) Ladd (1896, p. 3) said that mind “can never be defined.” (c) Washburn (1916, p. 17) said that mind is an “ultimate notion,” like “space,” and that “everyone knows what we mean.” Nevertheless, she offered a definition: “Consciousness is that which is present when we are either awake or dreaming, and which is absent when we are dreamlessly asleep.” (d) Dewey (1891, p. 2) had earlier rejected Washburn's kind of definition: “Consciousness can neither be defined nor described. . . . It cannot be defined by discriminating it from the unconscious, for this either is not known at all, or else is known only as it exists for consciousness.” (e) Rignano (1923, p. 359) said, “There is probably no word which has been more discussed, or whose meaning remains more obscure, than the word consciousness.” (f) Dunlap (1926) said that it meant both introspective observing of something and the thing observed by introspection. No wonder, then, that in a 1996 dictionary of psychology Sutherland said of consciousness “Nothing worth reading has been written on it” (p. 95).

Uttal's Approach

Uttal says, “Precise definitions require precise antecedent referents and conceptual anchors; unfortunately, such precision is notoriously absent when attempts are made to define mental terms” (p. 13). Granted, but

after noting the difficulty of defining mind, cognition, and mental terms, Uttal tries to remove this difficulty by offering no precise definitions (pp. 12–13), thereby ignoring rather than removing the difficulty. In offering no precise definition of mind, Uttal in effect says that he does not know what mind is. Therefore, he can have no justification for his assertions that mind is real but inaccessible and unanalyzable. The assertion that mind is real is crucial here, because unless it is justified, assertions about accessibility and analyzability can have no justification. Uttal asserts that subjective experience demonstrates that mind is real (pp. 2, 183), as noted earlier; but this assertion is problematic in three ways that are discussed in the following paragraphs.

Relevant subjective experience. Uttal's assertion is problematic because he does not specify what kind of subjective experience is relevant. Some kinds of subjective experience seem to justify asserting not that mind is real but that free will is real. Uttal would evidently not accept this justification of free will, because he asserts that the issue of free will “lies outside the domain of science” (p. xvi). He does not specify what science he is talking about, but two considerations are relevant.

First, free will does not lie outside the domains of some hermeneutic and phenomenological sciences. Admittedly, the designation of these sciences as “sciences” can be challenged, but the worldviews that underlie them are as adequate as mechanism (Pepper, 1942), which is the dominant worldview in mainstream psychology. On the basis of their own ground rules, they deserve the designation as much as mechanistic psychologies do on the basis of mechanistic ground rules. Therefore, using subjective experience as evidence that free will exists is as legitimate (or as illegitimate) as using subjective experience as evidence that mind exists.

Second, free will is outside the domains of modern behaviorisms and most cognitive psychologies, and it was outside the domain of Watson's science. However, mind was also outside the domain of Watson's science; therefore, subjective experience for Watson was irrelevant to the issue of mind as well as the issue of free will. In addition, the actual quality supposedly experienced in subjective experience was also outside Watson's science, as Washburn (1922) and Kantor (1933) not-

ed, and it is evidently outside Uttal's proposed new behaviorism. Thus, one of the major issues in classical psychology—the nature of what is experienced in subjective experience—is ignored rather than resolved. This restriction of scope may or may not be regretted, but in either case it is not necessarily a weakness. Analogies are that operant conditioning is outside the scope of psycholinguistics and the beauty of a rose is outside the scope of physics, but psycholinguistics and physics are nevertheless progressing nicely.

Relevance of subjective experience. A second problem with the assertion under consideration is that even if the kinds of subjective experience that are relevant to the reality of mind are fully specified, subjective experience cannot demonstrate that mind is real. It can demonstrate only that subjective experience is real, even if its reality is in hallucination, illusion, or imagination. In any case, however, when fully specified it can serve as a premise in an inference that mind is real.

Referent of subjective experience. The third problem is that Uttal's assertion is contradicted by his conclusion, discussed later, that mind is inaccessible. Accessibility of mind is implicitly a *required* premise in Uttal's argument that subjective experience demonstrates that mind is real (pp. 2, 183). It is required because if mental processes are inaccessible, a person cannot experience them.

Conclusions

Instead of proceeding to discuss something acknowledged to be incapable of precise definition, one should, I think, consider the possibility that the emperor is naked. The best conclusion, I think, is that mind is not a real process, a real thing, a real place, or any other such reality. It is only a word. It is too vaguely defined for theoretical and empirical purposes, but it is not a technical term and therefore it does not need a precise definition. Its vague definition is good enough for its real function—expository service in the titles of articles, chapters, and books, in which use of the word gives readers some preliminary sense of what they will find in the work.

DEFINITIONS OF TERMS

Uttal does not define several key terms, including *mind*, as already noted, and—in al-

phabetical order for ease of access to his subject index (which does not include the italicized items)—*awareness*, behavior, *behaviorism*, cognitivism, hypothetical construct versus intervening variable, *prediction*, *process* versus *mechanism*, *mentalism*, purpose as *intention* versus purpose as effect or obtained function (crucial on p. 38), and *self*. He notes the difficulty of defining some of these terms, but he seems not to realize the implication of this difficulty: Either the difficult terms are non-technical or they need to be narrowed by qualifiers.

Behavior

Uttal says that *behavior* is a descriptive approach (pp. 141, 142, 143, 187), but behavior is actually not an *approach* and not a *description*; it is a phenomenon to be *approached* and *described*. Uttal also says that attempts to define *behavior* are “fraught with many of the same kind of difficulties faced when one tries to define consciousness” (p. 164), and he concludes, “Perhaps, then, the best way to define behavior is simply to consider it to be nothing more than the sum total of the measures and operations that are used to measure it” (p. 165). The problem with this solution is determining what the “it” is that denotes which measures and operations belong in this class as contrasted with, for example, the class of stimuli.

Behaviorism

Uttal attributes to mentalism the issue of how inputs are transformed into outputs (p. 71) and to behaviorism the issue of how stimuli are transformed into responses (pp. 13, 68, 71, 113, 125, 148, 167). Neither issue is real because such transformations are not assumed to occur. The issues are not how inputs or stimuli are *transformed* into outputs or responses but how inputs or stimuli *lead to* (“elicit,” “occasion”) outputs or responses. Uttal's position may reflect his background in psychophysics and behaviorism. Transformations constitute the basic issue in psychophysics, according to Stevens (1951, pp. 30–31), but this issue refers only to transformations of the stimulus—“specification of all the transformations of the environment, both internal and external, that leave the response invariant” (p. 31) or, according to Thurstone (1927), invariant on average. Similarly, one of

the traditional issues in behaviorism, more often addressed conceptually than empirically, is the transformation of "potential" stimuli into "actual" or "effective" stimuli (e.g., Spence, 1956, p. 40; Watson, 1919, chap. 3–5), but the relation of actual or effective stimuli to responses is traditionally a "connection" rather than a "transformation."

Uttal refers to "behaviorism" as a "key term," but he says that it is "so festooned with emotional and historical connotations that . . . I am not sure what it means" (p. xvi). His confusion is evident when he refers to behaviorism and Gestaltism as "related psychologies" (p. 3). Actually, they seldom dealt with the same phenomena, and when they did, they dealt with them in philosophically incompatible ways.

I would suggest that the solution is to avoid talk about "behaviorism" and to talk instead about "stimulus–response learning theories" (or "theoretical behaviorism"), "methodological behaviorism" (i.e., theories in the tradition of Watson's scientific behaviorism), "cognitive behaviorism" (e.g., Tolman's "purposive behaviorism"), "behavior analysis" (i.e., theories in the tradition of Skinnerian behaviorism), and other explicitly specified kinds that seem relevant.

Mentalism

Uttal does no better in defining *mentalism* than behaviorism because he does not distinguish among the many ways that *mentalism* has been used. For example, many behavior analysts use the word *mentalistic* to refer to the mental behavior of theorists (e.g., Skinner, 1969, chap. 8). Moore (1984) used the word in this sense when he said that methodological behaviorism is mentalistic because it includes constructs that have no ontological reality; they are mentalistic constructs because they exist only in the minds of methodological behaviorists. This usage seems to me not very useful because it does not distinguish the mental activities of inventing concepts and inferring entities from the status of the invented concepts and inferred entities. Moore was evidently referring to "intervening variables" and "hypothetical constructs," as defined by MacCorquodale and Meehl (1948). These kinds of concepts are invented or inferred by means of mental activities, but the concepts themselves are generally understood to be

theoretical rather than mentalistic and to have a basis in ontological reality in that intervening variables are defined in terms of observable conditions and hypothetical constructs are defined as potentially observable behaviors or events (examples are given in the section "Types of Reduction"). Thus, in Skinner's and Moore's sense, the label *mentalistic* encompasses not only theories that just about any psychologist would label that way but also stimulus–response learning theories and Skinner's theory, which are not mentalistic by any other meaning of that word.

Like Skinner and Moore, Uttal does not distinguish between really mentalistic approaches and questionably mentalistic approaches. The really mentalistic approaches range from modern connectionism, which is quasiphysiological and mechanistic, to Piaget's brand of cognitivism, which is nonphysiological and consistent with dialectical idealism. The questionably mentalistic approaches include stimulus–response learning theories, which are mechanistic, and three "levels" of information-processing theories identified by Klahr (1973; Klahr & Wallace, 1976, pp. 5–6). Two of these levels (computer simulation and flow-chart "models") are mechanistic because the computer *is* a machine; they are consistent with theoretical or methodological behaviorism (Spiker, 1989). According to Klahr, the best example of the other level is Miller, Galanter, and Pribram's (1960) TOTE theory; it is consistent with pragmatism. Uttal does not mention the role of worldviews in the way psychological phenomena are defined, studied, and interpreted, leading to much confusion about differences between mentalistic and behavioral approaches. (For discussion of theories and worldviews, see Overton & Reese, 1973; Reese, 1986; Reese & Overton, 1970.)

FLAWED DISTINCTIONS

Several distinctions that are crucial to Uttal's arguments are not developed clearly in the book. The distinction between behaviorism and mentalism has already been discussed. Some other distinctions are discussed in the following paragraphs.

Types of Evidence

Uttal blurs the distinction between two kinds of evidence: direct observation and in-

ference (sometimes called indirect observation). Both kinds are fully legitimate in science when they satisfy their truth criteria, which according to Pepper's theory (1942, chap. 3) can be characterized, respectively, as interobserver reliability and circumstantial evidence. That is, the criterion for direct observation (which yields a "datum" in Pepper's terminology) is agreement among independent, qualified observers, and the criterion for inference is coherence of the inferred fact ("dandum" in Pepper's terminology) with a network of facts that were already known or accepted as true. By facts already known, I mean facts that came from prior direct observations that were reliable; by facts already accepted, I mean facts that came from prior inferences that are coherent. In other words, the network that verifies inferences can include "data" as well as "danda."

The distinction between the two kinds of facts is crucial for evaluating evidence about mental processes because direct observation cannot produce evidence about mental processes that is *reliable* (in Pepper's sense); therefore, the evidence must come from inference. Mental processes cannot be directly observed by any agent other than the one who performs them, and perhaps not even by that agent, as Uttal notes; therefore, Pepper's truth criterion for "data" cannot be met. Artifacts of brain activity, such as fluctuations in electrical charges and temperature, can be directly observed, but these artifacts are not interpreted as *mental* activity except perhaps in remnants of pre-20th century materialist theories in which brain activity secretes mind. Examples are the 18th century French materialist Pierre Cabanis, who said that the brain somehow "digests" sensory impressions and "secretes thought organically" (quoted in Kantor, 1969, p. 204), and the 19th century German materialist Karl Vogt, who said that "thoughts stand in the same relation to the brain as gall does to the liver or urine to the kidneys" (quoted in Gregory, 1977, p. 64).

Worldviews

Uttal seems to believe that mechanism is the only scientific worldview. He rejects pragmatism, but he does not mention two other nonmechanistic worldviews that Pepper (1942) evaluated as relatively adequate ("formism" and "organicism"), and he does not

acknowledge that all four of these worldviews have been associated with legitimate sciences—for example, mechanism with Hull–Spence learning theory, pragmatism with Tolman's cognitive psychology and Skinner's behaviorism, formism with Goethe's botany and Chomsky's generative grammar, and organicism with Piaget's theory (for references, see Reese, 1999b).

UTTAL'S POSITION ON ACCESSIBILITY AND ANALYZABILITY

On Uttal's page 1, accessibility means that mind can be "observed, measured, and then analyzed into its parts," but on page 2 his intended meaning is clarified: *Accessibility* means that mind can be observed, and *analyzability* means that it can be analyzed into its parts. Uttal stipulates further (p. 2) that the observation of mind is intrapersonal and that the accessibility issue is whether intrapersonal observation of mind involves the same techniques as interpersonal observation of phenomena. In this stipulation he does not distinguish between direct observation, which is personal, and indirect observation (i.e., inference), which is public and therefore objective. Thus, he does not distinguish between personal and objective knowledge, that is, everyday knowledge and scientific knowledge. The confusion is troublesome, because accessibility and analyzability are issues of scientific knowledge in Uttal's analysis.

Uttal notes that these issues are epistemological (pp. 2, 6), but he does not acknowledge that the observation and analysis of mind can therefore be conceptual, or inferential, rather than directly observational. In fact, he explicitly rejects accessibility via reduction to neurophysiological processes ("neuroreductive analysis"), conceptual analysis ("cognitive reductionism"), and inference based on self-report (p. 184).

I would say that mental processes exist ontologically and they can be presumed to have ontological consequences; otherwise, why would anyone be interested in them? The consequences might be mental products such as visual images or verbal concepts, which cannot be demonstrated to be reliably observable, or they might be overt behavior or artifacts, which can be reliably observed.

Therefore, even if the mental processes were not directly observable, some of their consequences might be directly observable, thus permitting indirect access to the mental processes via inferences based on their directly observed consequences.

Uttal says that mental events are inaccessible—this is one of his foundational “meta-principles”—yet he also says that inferences about mental events are usually incorrect. The problem with the latter assertion is that if mental events are inaccessible, that is, not directly observable, then obviously no one can use direct observation as a basis for knowing that inferences about mental events are incorrect. Consequently, the only evidence that inferences about mental events are incorrect must come from other inferences. Uttal spends considerable space attempting to demonstrate that introspection and other kinds of self-report are inaccurate, but how he knows they are inaccurate is sometimes unclear, given that he also says that these reports refer to something that is inaccessible. Furthermore, he uses the purported inaccessibility improperly. The proper use is to challenge the use of self-reports, but Uttal uses it improperly to challenge the possibility of cognitive psychology.

Uttal accepts Nisbett and Wilson’s (1977) conclusion that verbal reports are inaccurate reflections of mental processes when the reports disagree with “independent evaluations or behavioral measures” (p. 94). However, in accepting “independent evaluations or behavioral measures” as evidence about mental processes, he is acknowledging that mental processes are accessible; therefore, he cannot legitimately reject mentalism on the basis of inaccessibility to verbal report. Furthermore, verbal reports are themselves “behavioral measures,” and as such they are not necessarily less privileged than other behavioral measures. Finally, as noted earlier, accessibility of mind is implicitly assumed in Uttal’s argument that subjective experience demonstrates that mind is real.

AN ALTERNATIVE APPROACH TO MIND

A corollary of my earlier conclusion that mind is nothing and nowhere is that questions about its scientific accessibility and an-

alyzability are meaningless. The lack of progress in over a hundred years of scientific attempts to understand mind is consistent with this corollary. However, meaningful questions can be asked if *mind* is used only in its adjectival and adverbial forms, as Woodworth recommended in 1929.

Woodworth’s Recommendation

Woodworth (1929) said,

Since psychology studies activities, its terms are properly verbs, and adverbs. It needs one noun, *individual*, or *organism*, or the equivalent, as the subject of all its verbs; and, to be sure, it needs to name any number of objects that act upon the individual or are acted on by him. But the student will soon encounter an assortment of other nouns, names of activities and names of qualities, such as intelligence, memory, imagination, sensation, emotion, consciousness, behavior. All such nouns are properly verbs or adverbs, with “individual” as their subject. . . . We forget that our nouns are merely substitutes for verbs, and go hunting for the *things* denoted by the nouns; but there are no such things, there are only the activities that we started with, seeing, remembering, and so on.

Intelligence, consciousness, the unconscious, are by rights not nouns, nor even adjectives or verbs; they are adverbs. The real facts are that the individual acts intelligently—more or less so—acts consciously or unconsciously, as he may also act skillfully, persistently, excitedly. It is a safe rule, then, on encountering any menacing psychological noun, to strip off its linguistic mask, and see what manner of activity lies behind. (pp. 5–6)

Catania (1979, pp. 307, 338; 1992, pp. 302, 332) and Himeline (1980, p. 78) endorsed Woodworth’s recommendation. I disagree in only one respect: Psychology studies not only activities (behavior) but also their products, and therefore adjectives can be as useful as adverbs.

The Proposed Approach

The specific approach proposed here is fully consistent with either of the major versions of behaviorism—stimulus–response learning theory and behavior analysis—and therefore does not require modifying any behavioristic principles. The proposal is that the nouns *mind*, *consciousness*, and their synonyms do not need precise definitions because they are

used nontechnically in science, but that their adverbial and adjectival forms can be used technically to distinguish certain kinds of behavior from other kinds and certain products of behavior from other products. For this use the adverbs and adjectives need fairly precise definitions because otherwise the intended distinction is unclear.

The distinction I propose is between behavior and products that are called “mental” because they can never be observed by any organism other than the organism that emits them, as contrasted with behavior and products that are not “mental” because they are at least potentially available to outside observers. Only real behavior and products are observable; therefore, basing the distinction on type of observability requires that both the “mental” class and the “nonmental” class contain only real behavior and products. In this respect, my definition is consistent with Skinner’s (1974, chap. 7) position that private events are the same in kind and in lawful relations as externally observable behavior.

Implications

The mental class does not include any *overt* behavior and products because such behavior and products are potentially observable by others. Skinner (1957, p. 449) defined thinking to include overt as well as covert behavior, but according to the distinction proposed here, overt thinking is not mental. Similarly, mental behavior sometimes yields overt products, but the overt products are by my definition not mental because they are potentially observable by others. If an organism is isolated, all of its behavior and products may be unobserved by others. However, the only behavior and products that are mental by my definition are ones that cannot be made available to outside observation by use of available technology such as videotaping, heat sensing, or telescoping, because if they can be made available to outside observation—whether actually or only potentially—they are not mental. A corollary is that although physiological processes such as brain activity are classifiable as “behavior” broadly defined (Reese, 1970, p. 1), my definition of mental excludes them because available technology makes them potentially observable—via EEG and MRI records, for example, which are analogous to tracks in cloud chambers in physics.

My definition does not lead to an empty class of mental behavior and products because evidence supports the existence of instances of the class. This evidence includes self-reports. According to my definition, the mental class includes behavior and products that are not reported to have been observed by any person other than the person who apparently emitted them. The problem here is that self-reports do not provide direct evidence, contrary to the classical interpretation of introspective reports as immediate, direct, and (if the introspector is well trained) factually accurate (e.g., English, 1921; James, 1890, pp. 187, 194–197; Titchener, 1912, pp. 486, 508; Washburn, 1922). Self-reports are nowadays interpreted consistently with Watson’s interpretation of them as providing only indirect, correlational evidence (Watson, 1913a, Footnote 7, pp. 424–425; 1913b, p. 172; 1919, pp. 39, 42; 1920). Indeed, Ericsson and Simon (1993) cited Watson on the method (pp. 57–59) and implicitly endorsed his interpretation of it (pp. 372–373). Spence (1948) also explicitly endorsed this interpretation, but without attributing it to Watson.

Uttal, however, slips into the obsolete interpretation even while rejecting it, when he suggests that verbal reports access not “*what people think . . . [but] what people think they think*” (p. 102). As Watson pointed out, verbal reports directly access only what people *say* they think. Leeper (1951) also slipped into the obsolete interpretation in saying, for example, that in experiments on concept formation, “*introspective reports have disclosed* [*italics added*] that the subjects typically engage in an extremely active exploratory process, often formulating, testing, and discarding hypotheses within single trials” (p. 736) and “[in many studies] the experimenter neglected to collect reports to *show* [*italics added*] how the subjects interpreted their task and how they worked on it” (p. 737). This obsolete interpretation has become more prevalent in recent years, maybe because the new generations have forgotten why it was ever rejected.

A problem with the mental class is that it includes some behavior and products that are apparently unobservable even by the person who emits them. Obviously, the only kind of evidence about these kinds of behavior and products is indirect. Direct observation pro-

vides descriptions of phenomena, but these descriptions are not scientifically acceptable unless they are demonstrated to be reliable, that is, shown to be acceptable "data" in Pepper's (1942) sense. The only way to demonstrate reliability is to demonstrate agreement between observations by independent, qualified observers.

The foregoing considerations indicate that when direct observation of mental things is reported, the "data" cannot be demonstrated to be reliable, and when direct observation is not reported, "data" are not even available. Consequently, only indirect observation is even potentially available in these cases. If it is available, it can still produce evidence—not data, which come from direct observation—but the kind of evidence that Pepper (1942) called "danda," which is circumstantial evidence or, in a word, inference.

AN ALTERNATIVE POSITION ON ACCESSIBILITY AND ANALYZABILITY

The analyses thus far in this review indicate that meaningful questions can be asked about accessibility and analyzability, but that the questions are not the same as Uttal's because his questions are about the accessibility and analyzability of *mind*, which according to the preceding analyses is a reification. Rather, the meaningful questions are, "Are mental behavior and its mental products accessible; that is, can evidence about them be obtained?" and, if the answer is affirmative, "Are mental behavior and its mental products analyzable?" Both of these questions are in the domain of epistemology.

Accessibility

Uttal's analysis is ambiguous about the relation between accessibility and inference. In some places he says that accessibility does not encompass inference (pp. 18, 86), but in other places he says or implies that it does (pp. 3, 4, 96, 130–132). The latter position is the only real option for science, because if inference does not provide access to mental things, scientists can forget about mental things and leave them to philosophers and literary writers. Or if this pragmatic argument is questioned, one could cite Pepper's (1942, chap. 3) analysis of the nature of evidence,

which showed that "danda" can be as legitimate scientifically as "data." One could also cite an analogy to physics, in which unobservable subatomic particles are inferred from tracks in (ever more fancy) cloud chambers.

Given, then, that inference provides access to mental things, the question about accessibility can be answered affirmatively for mental behavior and mental products that have reliably observable concomitants, including self-reports as well as other overt products. If the concomitants are reliably observed, they can serve as premises in inferences about the mental phenomena in question. In contrast, for mental behavior and products that have no reliably observable concomitants, the answer to the question of accessibility is simply that this question has no meaning because it can have no empirical answer. It can have no empirical answer because no relevant evidence can be obtained.

Analyzability

When the accessibility question cannot be answered because relevant evidence is unobtainable, the question about analyzability is meaningless because nothing is available to be analyzed. However, when the accessibility question is answered affirmatively, the analyzability question is meaningful. When the analyzability question is meaningful, it can be asked in two ways, as Uttal notes, depending on the kind of analysis envisioned. Both kinds are reductive, but one kind is reduction to parts or processes that are in the same domain as the phenomenon that is being analyzed and the other kind is reduction to parts or processes that are in a different domain, such as reduction of psychological phenomena to physiological processes. An incidental problem in Uttal's discussion is that he does not maintain the distinction between the two kinds of reduction. Sometimes he says that the first kind of reduction (i.e., analysis) is not the same as reductionism or reduction to neurophysiology (pp. 2, 4, 85, 92) and sometimes he says that it is the same (pp. 4, 85, 91, 121).

TYPES OF REDUCTION

Within-Domain Reduction

When mental behavior or its product is being analyzed, the analysis is conceptual rather

than physical because mental behavior and products are inferred entities, given that reliable direct observational evidence about them is impossible. Inferred entities are “hypothetical constructs” in MacCorquodale and Meehl’s (1948) sense; that is, they are assumed to be real entities and are postulated on the basis of evidence, but they are not actually observed.

MacCorquodale and Meehl (1948) distinguished between hypothetical constructs and “intervening variables.” Briefly, a hypothetical construct is a hypothesized process or event, and an intervening variable is a theoretical concept that is defined by antecedents but does not name a process or an event. That is, a hypothetical construct is the name of a concrete process or event that although unobserved is hypothesized, assumed, or inferred actually to occur. In contrast, an intervening variable is an abstract symbol, and although it can be defined in terms of a concrete process or event, its referent is not this process or event. It has heuristic value but no concrete referent.

For example, in Hull–Spence theory the concept of “incentive motivation,” symbolized K , is an intervening variable defined as a mathematical function of the frequency and magnitude of rewards presented in a learning task (e.g., Spence, 1956, pp. 133–137). Spence proposed that incentive motivation reflects a particular hypothetical construct—an unobserved conditioned consummatory response called the “fractional anticipatory goal response,” symbolized r_g , together with the stimulus feedback it produces (s_g). K was not assumed to have any existence outside the theory, but r_g – s_g was assumed to be an actual response and its actual product. Another example is Hull’s (1943) concept of “drive”; it is an intervening variable defined not in terms of physiological states but in terms of prior deprivation (e.g., hunger) or in terms of strong stimulation (e.g., pain). Similarly, Hull’s concept of “excitatory potential” was an intervening variable defined in terms of other intervening variables: drive, habit strength, inhibitory potential, and so forth.

MacCorquodale and Meehl’s (1948) distinction was criticized by Bergmann (1951), praised by Rozeboom (1956), and ignored by Hacking (1983), and it is used inconsistently

by Uttal. Uttal sometimes uses “hypothetical construct” (sometimes with variant words) consistently with MacCorquodale and Meehl’s definition (pp. 17, 18), but he sometimes uses that term to mean “intervening variable” (pp. 41, 42, 51, 72–73). For example, he notes correctly that intervening variables are not posited to identify real underlying mechanisms, but he weakens the point by saying that they are “*essentially neutral*” in this respect (p. 155; italics added). Another example is that he says, “Hull was a prolific definer of inferred internal states and variables (e.g., excitatory tendency)” (p. 42); but in fact Hull made no formal use of inferred internal states and variables in his theory, and as mentioned above, “excitatory potential” was an intervening variable, not an inferred state. Uttal also uses the phrase “unverifiable intervening variables” (p. 41), but intervening variables are theoretical concepts and the only ways they can be verified are (a) by demonstrating that their antecedents actually exist or (b) by testing and confirming predictions they lead to. However, the first way is better called “adequately defined” than “verified” and the second way is better called “useful” than “verified.” A final example is that on page 156 Uttal reverses the roles of hypothetical constructs and intervening variables in saying that the former are involved in explanations and the latter in descriptions (intervening variables are also called descriptive on p. 155). Actually, hypothetical constructs are hypothesized parts of a description and intervening variables are parts of a theoretical explanation.

Therefore, because mental behavior or its product is an inferred entity—a hypothetical construct that cannot be directly observed—it cannot be literally analyzed because literal analysis means literally taking something apart. Mental behavior or its product can be taken apart only conceptually, that is, theoretically or hypothetically. However, analysis into conceived or hypothesized parts is always possible; therefore, the answer to the within-domain version of the analyzability question is always affirmative when the answer to the accessibility question is affirmative. Of course, such an analysis may or may not be scientifically useful, depending in part on the absence of circularity. An analysis is useful if it goes beyond the phenomenon in question by

incorporating other phenomena and generates predictions about new phenomena (e.g., Allport, 1955, pp. 8–9; Kuhn, 1977, pp. 321–322; Plomin, 1987, p. 364; Robinson, 1979, p. 262).

Between-Domain Reduction

The second kind of reduction is always to a domain that is purportedly “more basic” than the one in question. For Uttal this kind of reduction is usually from psychology to neurophysiology. Uttal sometimes designates this kind of reduction as “ontological” (e.g., pp. 86, 88, 188), but this designation is misleading because, like within-domain reduction, between-domain reduction is conceptual rather than physical when the topic is mental behavior and its products. Therefore, contrary to the implication of the designation “ontological,” psychological phenomena cannot be literally reduced to physiological phenomena; at most they can be explained by reference to physiological laws and facts.

Uttal also, however, sometimes refers to this kind of reduction as explanatory reduction (pp. 19, 154), which strongly implies that he recognizes that it is in the epistemological domain, that is, conceptual or theoretical. Indeed, he explicitly notes that theory is involved in this kind of reduction (e.g., p. 154). The actually attempted reductions of this kind have usually been not to actual laws and facts of physiology but to theoretical concepts with names that sound physiological. Uttal (p. 17) cites Hebb’s (1949) “cell assemblies” and “phase sequences” as examples of physiological concepts, but they actually have no basis in real physiology and hence are psychological concepts disguised by their names as physiological. Hull’s (1943) concept of “afferent neural interaction” is another example of such a concept; modern connectionism and other cognitive sciences provide many other examples.

Despite the theoretical basis of between-domain reductions, the ones that have so far been accomplished have empirical as well as theoretical justification. The empirical justification is an obtained correlation between mental phenomena such as memory and neurophysiological artifacts revealed by, for example, EEG and MRI. Thus, between-domain reductionism is not entirely theoretical.

Role of Reductions in Psychology

Uttal says, “The ideal goal of a science is *ontological reductionism* in which the phenomena at one level of discourse are explained in terms of mechanisms and processes at more microscopic levels” (p. 88). If this premise is denied, as I believe it should be for reasons given elsewhere (Reese, 1996a, 1996b), psychological science is not weakened in any way even if between-domain reduction never occurs. That is, psychology as such—whether behavioral or cognitive—does not need to be reduced to neurophysiology or to anything else, though as I have noted elsewhere (Reese, 1996a, 1996b), some psychologists have felt a need for this kind of reduction.

In contrast, psychological science would be considerably weakened if the within-domain kind of reduction failed, as virtually all psychologists have recognized. For example, Skinner (1950; 1974, pp. 240–241; 1978, p. 123) explicitly rejected the need for between-domain reduction, but he embraced the need for analysis, as in the hallmark analysis of operant behavior into a three-term contingency of discriminative stimulus, operant response, and contingent stimulus. Even Karl Marx, whose philosophy (dialectical materialism) and socio-historical-economic theory (historical materialism) are holistic, believed that understanding the whole must begin with conceptual analysis into hypothesized parts, followed by conceptual synthesis of the whole from the hypothesized parts (K. Marx, 1939/1973). In Marx’s approach, analysis must be conceptual, or epistemological, because ontological analysis, such as physical dissection, would destroy the whole and preclude synthesis. Analysis of mental things is necessarily epistemological for the same reasons.

UTTAL’S PROPOSED NEW BEHAVIORISM

An extremely serious problem with Uttal’s book is that his proposed “revitalized psychophysical behaviorism” is unlikely to serve the goals he has in mind. He wants his new behaviorism to have the following characteristics: psychophysical, mathematically descriptive, neuronally nonreductive, experimental, molar, “*empiricist₁*” and nativist, “*empiricist₂*” and rationalist, and antipragmatic (p.

138). Each of these characteristics is examined in the following subsections, with the same headings as Uttal's subsections.

Psychophysical

Uttal wants the methodology of his new behaviorism to be psychophysical insofar as possible because psychophysics worked. However, in his discussion he makes the classic mistake of interpreting psychophysics as the study of relations between stimuli and "sensory processes" or percepts: "The measures of physical science . . . served as precisely defined standards against which the perceptual response could be calibrated" (p. 139). Actually, psychophysics was always the study of relations between stimuli and overt behavior, as Uttal acknowledges on the same page, but he interprets the overt behavior as "physiological discrimination factors" (p. 139). The observed relations were only inferentially related to relations between stimuli and sensory processes or percepts, or between stimuli and physiological processes. Granted, on the next page Uttal says that "No hypothetical mental constructs or pseudophysiological explanations were a necessary part of the traditional psychophysical agenda" (p. 140). Nevertheless, the word "necessary" in this quotation is a weasel word that robs the assertion of any definitive meaning—as indeed it should because his comments on page 139 show that the hypothetical mental construct of perception and pseudophysiological explanations are still part of the psychophysical agenda. Consistently with this point, Uttal refers to psychophysical graphs relating stimulus features to changes in "the *perceived* response" (p. 140; italics added).

Uttal argues further that the simplest psychophysical method—signal detection—should be used because "[it requires] no introspective judgments, no interpretative self-analysis, just the simplest possible response to a well-defined stimulus. Whether it was a yes-no procedure or a multialternative forced-choice one, the subject was permitted only a simple, Class A, discriminative response" (pp. 139–140). This argument is problematic in several ways. One is that, judging from Uttal's discussion of Brindley's distinction between Class A and Class B responses (pp. 101–102), Brindley made the classic mistake about the nature of the response. Ac-

cording to Uttal, Brindley's "main point was that Class A observations are the only ones that at least modestly assured the theoretician that the verbal report is validly describing or measuring the true mental (i.e., perceptual) processes that account for the stimulus-response relations" (p. 102). In fact, Class A responses—and for that matter, Class B responses as well—are not *observations*, not *descriptions*, and not *measures* of mental processes; they are only verbal *responses* (cf. Thurstone, 1927). As already mentioned, they were indeed sometimes interpreted as direct reflections of mental processes, but this interpretation is inferential (e.g., James, 1890, pp. 545–549; Kantor, 1969, chap. 35; Thurstone, 1927).

A second problem is Uttal's reference to Class A responses as "discriminative" responses and, earlier, as "essentially pure discriminations of identity or equality" (p. 101). Skinner (1974) pointed out that *discrimination* is not a response, not a property of a response, and not a process; it is an unnecessary interpretation or inference. The observed Class A response is a simple verbal behavior such as saying "yes" or "no," and calling it discriminative or a discrimination is obviously a theoretical interpretation of it.

A third problem is Uttal's interpretation of Class A responses as reflecting "essentially pure" discriminations. Psychophysical research clearly showed that the inferred discriminations were not pure and not even "essentially pure." Examples include criterion effects in signal-detection research, differences between thresholds in ascending and descending series (Bourbon, 1978), and effects of background stimulation in adaptation-level research (Helson, 1964). Nonpsychophysical examples include effects of verbal labels in research on the acquired distinctiveness and equivalence of cues (Dollard & Miller, 1950, p. 104) and effects of prior training in transposition research (Reese, 1968).

A fourth problem reflects the very simplicity of Class A responses. This problem is revealed by analogy to the sterility of several generations of research on memory that followed Ebbinghaus's attempt to eliminate meaning from memory.

The upshot of Uttal's argument is that psychophysics was successful and therefore its methods should be preferred in the new be-

haviorism. One problem is that one could as well argue that the methods of physics and chemistry work and therefore those methods should be preferred in psychology, even though the actual methods of physics and chemistry are inapplicable in psychology. Another problem is that the original aim of psychophysics was to study sensation and perception, and this aim was not addressed by psychophysical methods (Kantor, 1969, chap. 35). For the same reason, psychophysical methods are useless for studying any other mental phenomena, regardless of whether the study is behavioristic or cognitive.

Mathematically and Behaviorally Descriptive

Uttal says that the goal of his new behaviorism is description rather than explanation because the internal structure underlying behavior is inaccessible (pp. 141–143). He says that theories, which he sometimes calls models, are “spurious if they are presented as reductive explanations” but not “if description is all that is being claimed,” provided that the description meets the criterion of “goodness of fit” (p. 143). I see five major problems here, which are discussed in the following paragraphs.

Description versus explanation. One major problem is that Uttal, together with many other scientists and many philosophers, does not distinguish clearly between description and explanation. I have discussed this distinction elsewhere (Reese, 1999a). The crucial points are that description and explanation have different roles and different purposes, are evaluated on different grounds, and when they are problematic, they are problematic in different ways.

Theories versus models. A second major problem is that Uttal, again together with many other scientists and many philosophers, does not distinguish between theories and models. When the distinction is made, a device that is intended to be explanatory is called a *theory* and a device that is intended to be descriptive is called a *model* (Overton & Reese, 1973; Reese & Overton, 1970; and sources they cited). The distinction may seem to break down in some cases, as when a model guides the collection, analysis, and interpretation of data (Overton & Reese, 1973), but in these cases the model can be said to have evolved into a theory. The distinction is not merely termi-

nological, however. According to the distinction, theories are epistemological devices and models are ontological devices. A theory is judged to be true or false depending, for example, on whether the predictions it generates are verified. A model can be useful to some degree or useless, but it cannot be true or false because it is only a metaphor, which is what *model* means in science (e.g., Sutherland, 1996, p. 278).

Two subproblems follow from the problem under consideration: (a) Goodness of fit is not a criterion for the truth of any theory, except in the sense that a prediction “fits” an observation, which is usually called the “correspondence” theory of truth (e.g., Pepper, 1942, pp. 221–231). (b) Goodness of fit is not a criterion for the usefulness of any model because the only way we can know ontological reality is by means of models, as Kant showed in his distinction between *noumena* and *phenomena* (e.g., Prosch, 1964, chap. 5). Relevant to this point, Uttal does not specify what it is that the model might fit.

Mathematics as description. A third major problem is that Uttal believes that mathematics is descriptive (pp. 89, 141, 187), although he acknowledges that it is really only a descriptive *tool* (p. 141). In either case, his insistence on the use of mathematics in descriptions is not always justified. He says that “mathematics works well in doing what it does and what it does superbly is to describe” (p. 142). Earlier on the same page, however, he undermines the claim to superbness in saying that the use of mathematics sometimes imposes an order that does not exist in the phenomenon. A noteworthy point here is that the use of mathematics in describing a phenomenon constitutes using a mathematical “model” of the phenomenon, and “goodness of fit” can be evaluated by comparing this model with some other model, such as a holistic one. However, the comparison cannot indicate that the models are equivalent or different in their fit with reality; it can indicate only that the models do or do not fit each other.

Behavior as description. A fourth major problem, noted earlier, is that Uttal says that *behavior* is a description, but does not say what it describes. What he apparently means is that the observation of behavior does not constitute observation of underlying processes or

mechanisms. I am not aware of anyone—behaviorist, cognitivist, mentalist, or philosopher—who would challenge this point.

Nature of the new behaviorism. A fifth major problem is that Uttal's recommendations in the section under consideration would transform current behaviorism, which is a "natural science," into a "natural history," which is a purely descriptive enterprise—"mere bug-hunting" in Toulmin's apt phrase (1953, p. 54). The aims of natural science go beyond description of regularities to explanation of the regularities (e.g., Bergmann, 1957, p. 79; James, 1907/1981, pp. 30–32; M. H. Marx, 1951, pp. 5, 6; Pepper, 1966, pp. 265–266; Reese, 1999a; Skinner, 1931, 1953, pp. 13, 15–16; Spiker, 1986; Toulmin, 1953, pp. 44–56). To paraphrase Kant's famous aphorism, explanations without facts (descriptions) are empty, and facts without explanations are blind (Kant, 1787/1965, p. 93).

Neuronally Nonreductive

The gist of the nonreductionist characteristic is that neurophysiology is so complex that reduction of psychology to neurophysiology cannot work and the hope for this reduction should be given up. I agree with this point, but I would add that some psychological phenomena have been successfully "reduced" to neurophysiological processes. That is, although psychology as a whole will probably never be reduced to neurophysiology, smaller scale reductions are not only possible but have been accomplished (for examples, see Reese, 1996a, 1996b). As Uttal says in the psychophysics section, "In a few special cases . . . the mechanisms underlying the [psychophysical] transform were sought in some fortuitously related physiological or anatomical knowledge" (p. 140).

I would also repeat here a point made earlier—that psychology does not need this kind of reduction even on small scales. When small-scale reductions are accomplished, psychologists may consider them to be important breakthroughs or may consider them to be only interesting curiosities. In any case, as Spiker (1966, p. 48) said, until successful reductions occur, "we do not have much to fuss about."

Experimental

The strong implication of the title of this section is misleading because Uttal equates

"experimental" and "empirical," contrasts it with "speculative," and does not distinguish between experimental control and statistical "control." The problems are: (a) Experimentation is of course empirical, but not all empirical research is experimental. For example, if Uttal is correct in saying (p. 188) that experimentation is a *sine qua non* of any science, then astronomy, for example, is not a science. If, however, he means "empirical," then the scientific status of astronomy is saved. (b) I have never heard of any psychologist who advocated that psychology should be speculative rather than empirical. (c) Uttal refers to "good statistical practice that prevents artifacts and confounds from inadvertently biasing the results" (pp. 145–146); but actually, such effects may be controllable experimentally but cannot be controlled statistically (Reese, 1997).

Molar

Uttal says that the antithesis of reductionism is holism (p. 146), but actually the antithesis of *ontological* reduction is nonreduction and the antithesis of *epistemological* reduction is synthesis. Epistemological reduction is conceptual analysis; synthesis is construction of a whole from parts (here, conceptual construction of an inferred whole from conceptual parts).

Uttal wants his behaviorism to be holistic or molar, and he says that "From the very beginning, behaviorism has been explicitly a molar science. It has been concerned with the specific measurable actions of the subject, not the components of that action" (p. 146). Actually, behaviorism was analytic from the very beginning rather than molar in the sense Uttal wants, which as he notes (p. 147) is closely related to Gestalt psychology. He says that "a satisfactory modern behaviorism . . . would encompass and incorporate many of the holistic and phenomenological aspects of Gestaltism" (p. 147). In fact, however, Gestalt psychology was mentalistic; therefore, behaviorism would not adopt any of its "aspects." However, behaviorists using their own methods might discover principles that turn out to have the same empirical content as some Gestalt principles. If so, behaviorists would presumably accept the Gestalt phenomena but not the Gestalt interpretations of them.

Uttal also says that “if one is to accept the evidence so far presented that analysis of cognitive processes is as limited as is their accessibility, then the logical conclusion must be that an emphasis on the overall, molar response should be a mandatory feature of 21st-century behaviorism” (p. 147). This “logical conclusion” is actually a non sequitur; Uttal is saying that if one accepts that (unobserved) cognitive processes are unanalyzable, then one must accept that (observed) behavior is unanalyzable.

Finally, Uttal attributes to physics an analogy of behavioral holism: “A physicist would assert that the individual particles that make up a gas can be collectively characterized by the holistic term ‘pressure’ ” (Footnote 1, p. 146). Actually, “pressure” in physics is a property of a collection of particles and—most important here—it theoretically results from the behavior of individual particles. Furthermore, molar behavior is not analogous to a property of a collection of subbehaviors. Therefore, the analogy is not persuasive.

Empiricism₁ and Nativism

Uttal cites an analysis that he developed in an earlier book, in which nativism and “empiricism₁” are the poles in the nature–nurture issue. He says that “A modern behaviorism must accept the interplay of both maturation and experience in determining our responses” (p. 148). True. Uttal adds that few psychologists endorse either extreme and that the issue is therefore “more or less a red herring” (p. 149). True again. However, he also says that nativism and empiricism₁ are not a dichotomy but rather are ends of a continuum (pp. 147–148). I have not read Uttal’s earlier analysis, but nature and nurture usually refer to two kinds of influence on behavior that are a true dichotomy; the continuum Uttal has in mind is presumably the extent to which nature and nurture influence behavior. Similarly, nativism and empiricism usually refer to two mutually contradictory philosophies, which therefore cannot be ends of a continuum; furthermore, because they are philosophies, they are relatively independent of empirical evidence (e.g., Overton & Reese, 1973; Reese & Overton, 1970).

Finally, if recognizing that both nature and nurture influence behavior is “an eclectic compromise” between nativism and empiri-

cism, as Uttal says (p. 148), then this recognition is a scientific mistake because, as Pepper (1942) said, philosophical eclecticism is confusing when it is taken seriously. Fortunately, however, no eclectic compromise is involved because when both nature and nurture are acknowledged to influence behavior, both extremes—nativism and empiricism—are rejected and some other philosophy is accepted.

Empiricism₂ and Rationalist

Uttal says that in his earlier analysis, “empiricism₂” and rationalism are the poles in the “direct-mediated” issue. He says that the extreme positions are contradicted by the evidence but are endorsed by few psychologists and therefore that the distinctions are also “more or less a red herring” (p. 149). All of the points in my criticism of Uttal’s analysis of the nature–nurture issue and empiricism₁–nativism are applicable here.

Uttal concludes that “a general eclecticism may be among the most important criteria of any new psychological position. Eclecticism requires any science to keep an open mind toward new developments as well as ancient controversies” (p. 149). I agree with what he means to say, but it is about open-mindedness rather than serious eclecticism.

Antipragmatic

On page 149 Uttal refers to the philosophy called pragmatism, citing its founders Charles S. Peirce and William James, but he then describes and rejects a kind of pragmatism that is unrelated to pragmatic philosophy, which is described in Hayes, Hayes, Reese, and Sabin (1993) and many other sources. Uttal thinks he is attacking pragmatic philosophy when he says,

We should not attempt to build a façade of false knowledge simply to achieve practical goals for human factors engineering, education, psychotherapy, or for some wistful humanistic longing, no matter how worthwhile they may otherwise be. When a question of such fundamental importance as the one of accessibility is asked, we should not be misled by the need or desire to solve some important extra-scientific problem to impute or infer that which is unobtainable. (p. 149)

Actually, the pragmatic philosopher of science Larry Laudan (1977) made essentially

the same points. Thus, Uttal's conclusion in this section—that “any revitalized behaviorism must be based on an antipragmatic and proscientific foundation” (p. 150)—is based on a false contrast.

SUMMARY

Uttal's proposed new behaviorism seems to be regressive rather than progressive, a natural history rather than a natural science, and—most important—based on questionable ideas about the nature of science. In regressing to psychophysics, it is also atavistic, and in incorporating certain mentalisms (e.g., “cognitive decisions” and “logically mediated . . . factors” on p. 188), it is not entirely behavioral. In this respect, however, no version of behaviorism has been entirely behavioral.

Another problem is that Uttal's belief that a new behaviorism is needed is based on incomplete understanding of mentalism, cognitivism, and behaviorism and erroneous conclusions about the accessibility and analyzability of mental phenomena, the nature of description and explanation, the distinction between direct observation and inference, and so forth. In short, Uttal does not settle *The War Between Mentalism and Behaviorism*, he abandons it; and he settles the issue in the subtitle—*On the Accessibility of Mental Processes*—by rejecting inference as a source of access.

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